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ABSTRACT

IN A PAPER GIVEN AT THE 1969 AMERICAN PSYCHOLOGICAL ASSOCIATION MEETING, THE AUTHOR POINTED OUT THAT THE APPARENT COMMITMENT OF OUR SOCIETY TO THE EDUCATION OF THE YOUNG WILL BE OF QUESTIONABLE VALUE UNTIL THE ENERGIES AND EFFORTS OF EDUCATORS FIND A MEANINGFUL DIRECTION. RIGHT NOW, THE FLURRY OF ACTIVITY IN EDUCATIONAL REFORM FOR THE VERY YOUNG IS MORE CONCERNED WITH ACTIVITY THAN RESULTS. FOR EXAMPLE, THE HEAD START PROGRAM IS RECEIVING MORE AND MORE AMPLIFICATION IN SPITE OF THE MIXED EVIDENCE CONCERNING ITS SUCCESS. ALSO, THE QUESTIONABLE TACTIC OF IMPOSING MIDDLE-CLASS LIFE ON THE ECONOMICALLY DEPRIVED TO ATTUNE THEM TO OUR MIDDLE-CLASS SCHOOLS CONTINUES AS ONE OF THE MOST WELL-ADVERTISED EFFORTS OF CONCERNED REFORMERS. PSYCHOLOGISTS CAN BE MOST USEFUL IF THEY WILL ASSUME ROLES AS SETTERS OF GOALS, SOURCES OF INNOVATIVE EDUCATIONAL IDEAS, REPOSITORIES OF KNOWLEDGE, AND GATHERERS OF INFORMATION. THE CURRENT STATE OF KNOWLEDGE OF BASIC PSYCHOLOGICAL PRINCIPLES FOR EDUCATIONAL REFORM IS INDEED SAD. IT IS IN RESEARCH THAT PSYCHOLOGISTS HAVE THE MOST TO CONTRIBUTE. THIS RESEARCH COULD BEST BE CARRIED OUT IN EXPERIMENTAL SCHOOLS WITH VARIOUS INNOVATIVE TECHNIQUES BEING MEASURED AGAINST EACH OTHER. (MH)

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Early Learning and Compensatory Education: Contribution of Basic Research.

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A paper read at the meetings of the American Psychological Association August 31, 1969.

Every once in a while, a culture commits itself to an idea. We seem to have decided that the hope of the future is in the education of the very young the Headstart program which we continue and amplify in spite of mixed evidence of its success, the establishment of an Office of Child Development, the implications of the President's message on welfare for vastly enlarged day care, the increasing interest of professional people in early childhood, all testify to our decision -- partly theoretical, partly political, and partly magical -- to study and to educate the infants. More -- we have an analogy. Often over the past two weeks one could hear, "If we can put a man on the moon, surely we can save the children." It is a revealing and instructive analogy. What did it take to put man on the moon? Scientifically, Newton's laws of motion (and a little help from James Clerk Maxwell), 24 billion dollars worth of technology, and a firm goal -- the moon. With proper respect for the advances of the social sciences over the last years, we have not had our Principia and, more vividly, we do not know where we are going. Consider for a moment the variety of ends that have been proposed for early childhood education -- improvement of maternity, extension of group care, embedding of child care in a comprehensive system of social change, cognitive change, affective change, assimilation into the mainstream, protection of indigenous culture, early reading, early ecstacy, early political sense -- the divergency and confusion of educational goals for the child makes it no exaggeration to say that, if we are to follow the example of Apollo 11, we must head into space with neither Newton nor the moon.

The truth is not altogether bleak, however. The concern with infancy and the education of babies represents a too-long delayed recognition that the problems of the nation posed by poverty and racial discord require attention from all of us and hard thought. Something must be done. But the very importance of early childhood education should warn us to be wary, especially about the different jobs we do. Psychologists are n-headed men like other professionals and, in the field of early education, the psychologist may serve as a setter of goals, as a source of innovative educational ideas, as a repository of knowledge, and as an information gatherer. I believe that so elemental a reminder is necessary because of a remarkable tendency for the several tasks to become conflated. Information that we gather becomes -- in ways that no one has systematically analyzed -- support for our system of educational values. Examples of the tendency abound but let me try to form a fictitious case. If we believe that early reading is a social value, the finding that children of two can read becomes somehow relevant to and supportive of the initial belief that early reading is valuable.

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I do not mean to suggest that psychologists keep their opinions about values and directions to themselves but I, with many others, most warmly urge that we do not pose as especially competent in matters of ultimate value. Psychologists do not have, more than other citizens, the right to decide about the directions of our lives together. In paricular, I am concerned about the game that can only be called black-fixing -- the usually implicit, sometimes explicit assumption that all we need do is modify the environment of poor black people to make it more like the environment of middle-class whites and the world will be made whole. I am afraid it is a dominant principle of remedial education nowadays and I am even more disturbed to see psychologists maintaining that they can document the virtues of such cultural intervention. But, beyond this specially crucial matter of value, it is important to raise the general issue of the competence of experts. To the degree that we confuse -- for ourselves and others -- the several tasks psychologists have in early education, to that degree we will often invite and will deserve false expectations, disappointment, and resentment.

Need we be as gloomy as I indicate? Well, let's go to the psychologist as a repository of information and ask just a few questions about early education.

1) What are the differences in handling and outcome between group day-care of young children, fulltime boarding care, and home rearing?

2) How do such differences vary with age, with prior environment, with training of caretakers, with physical setting; to name only a few?

3) What reliable and predictive measures of behavior before the second birthday can we use to study our educational procedures?

4) How does the evidence look on training the child to be cognitively competent in comparison with training him to be emotionally sensitive?

5) What are the implications of instituting educational changes without modifying the political and economic context?

Again, I rush to say that we are not totally in ignorance about these issues but, at the very least, the evidence of basic research is insufficient to make clearcut defensible decisions about any of the questions I posed. As a colleague has noted, our evidence about early education is of the sort that is adequate if you are deciding whether to package your new cereal in red boxes or blue boxes but hopelessly inadequate if your task is fixing tolerances on a new suspension bridge. In compensatory education, we are about to build bridges with cereal-box technology. And, for many complicated reasons, the world of educational reform will not wait for us and (in my opinion at least) it should not. What needs emphasis is that the basic research that is currently available is most valuable as it inspires and provokes sensitive and talented educational innovations, rather than as it prescribes solutions.

When we come at last to the place of psychologist as information gatherer, the cloud lifts a bit. Surely, our special contribution to educational reform will be as designers of research that will, over a time-scale fundamentally different from the time-scale of reform, begin to provide the firm answers we require. Perhaps the most important

function of research is to protect good ideas and the diversity of ideas in compensatory education nowadays suggests that some of them, at least, need protection.

Messick will talk about evaluational research. In the next minute or so I would like to propose an arrangement for research on early childhood which may also hold promise for a more efficient attack on the questions facing us. One way of expressing the research dilemma is that we need to bring the precision and objectivity of the laboratory into realistic settings, to supplement essential small-scale laboratory work and essential demonstration and innovation with large-scale rigorous research. Morrisett and I have explored this possibility for som time now and I believe that the primary need is for a few experimental schools -- not demonstration schools where a new idea is installed full-sweep -- but schools where experiments, both in the long term and in the short term, are carried out. There can only be several such schools -- there are preciously few educational researchers available -- but there probably ought to be more than one to insure some diversity. Early childhood education -- because the institutional forms are not yet set and because the research problems are exquisitely important -- would be an ideal setting for experimental schools. What is proposed here is not the same old limp appendage of research on an operational program; rather, true experimental schools would have two staffs -- one for instructional operation and one for research. Research questions would be systematically addressed; for example, the schools would permit varying more values of a dimension of treatment than the something-vs-nothing designs now forced upon us by limited funds and limited control. Curriculum innovation, teaching techniques, power arrangements in class, administrative structures, all could be studied realistically and with sufficient control.

Let me cite an example. Particularly with very young children, a great deal of educational theory and a number of recent proposals rest on assumptions about the child's response to richly varying materials and about the educational effectiveness of such materials with and without adult tuition. In an experimental school (or school system if problems of pupil placement can be managed), a number of classrooms varying in the admixture of people and things, and with realistic budgets, could be established. The research would not aim, as the temptation may be, to validate a procedure and invalidate another but rather to permit the careful analysis of the dimensions of intervention and their effects. In spite of much good work by many good people, dimensional analysis is far too rare for basic research to be closely relevant to the design of educational change. I recognize full well the difficulties such a proposal raises for issues of community participation, relation with teachers and educational administrators, and connection with university researchers. The problems, as a matter of fact, may be irretrievably difficult but the attempt to establish research schools of this sort seems necessary if we are to avoid the frustrating disconnection between basic research and educational innovation.

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